gardens within the immediate vicinity of the metropolis." Both were strictly metropolitan institutions, and both were essentially educational establishments, differing only in scope. Various exigencies have at times modified, and even obscured, their primary object, but each has, so far as circumstances permitted, devoted itself to educational work. The extent and value of the services they have rendered, and continue to render, in this direction are fully detailed in the work under review.

Kew, on the other hand, can only be termed a *London* botanic garden in a strictly limited sense. As a national institution it has a much wider field, and its activities are on a correspondingly broader basis. Unlike the other gardens, it does practically no direct educational work, but " stands out prominently as a centre of botanical research, and as the cradle of botanical enterprise in India and the Colonies."

The principal aspects of Kew work are touched upon, but the limits of space have compelled the writer to treat them by way of illustration rather than exhaustively. No reference is made to the horticultural or ornamental side of Kew. While detracting in a measure from the completeness of the sketch, the omission is the less to be regretted because of the growing tendency on the part of the general public to regard this feature as fundamental rather than incidental, and to look upon botanic gardens as places of recreation rather than as scientific institutions. Mr. Perrédès's work, by directing attention to the conspicuous part that the London botanic gardens have played in the scientific and material progress of the nation, should go far towards removing the reproach that our botanic gardens are better understood and more appreciated abroad than at home, a reproach which gains point from the fact that the papers under notice were contributed to an American journal, and are only available in this country at second-hand.

The work is well illustrated, and contains a copious bibliography.

THE NATIONAL PHYSICAL LABORATORY.[1]

THE recent discussion of the affairs of the National Physical Laboratory in Parliament, and the appointment of a departmental committee of the Treasury to inquire into the working of the laboratory, with special reference to its alleged " competition with private establishments," have tended to produce amongst the newspaper-reading public an impression that the institution was not being carried on satisfactorily. It may be useful, therefore, to state in a few words what is really the position of affairs.

The laboratory was established in 1899 to serve as an independent testing authority, and to carry out researches into the properties of materials which, while necessary for the advance of the industries of the country on scientific lines, are generally too extensive and laborious to be undertaken by private individuals. It was not anticipated that it would ever be necessary to compete with the existing private institutions in the testing of materials, but nothing in the Royal Society's scheme on which the laboratory was founded limited its testing powers. Once it was equipped and staffed, the desire of industrial firms to have their materials tested by men who had already made names for themselves in the scientific world appears to have led to much work of this kind being sent to the laboratory, and it is difficult to see on what grounds it could be

refused. Whether it is to be undertaken in future or not the Treasury Committee must decide.

With regard to the research work of the laboratory, there can be no two opinions. A glance through the two works under notice is sufficient to show how well it is fulfilling its task. Dr. Stanton's work on the resistance of iron and steel to reversals of stress is supplying information urgently needed, and with Dr. Carpenter's work on the structure of high-speed tool steel and on the properties of iron-nickel-manganese-carbon alloys is constituting the laboratory *the* authority in this country on the properties of the materials used by mechanical engineers. Mr. Paterson's investigations on light standards and glow lamps, communicated to the Institution of Electrical Engineers in January, supply gas and electrical engineers with information of the greatest value as to the relative merits, or rather demerits, of the various standards of light. Mr. Campbell's researches on the properties of the paper and cellulose used in telephone cables, on insulating materials suitable for high temperatures, and his hysteresis research, all bear intimately on the electrical engineering industry, as does Dr. Caspari's work on gutta-percha and balata. Dr. Harker's new bench-mark 1710° C. for the melting of platinum will serve as a starting point for a revision of all our high temperature melting points, and will introduce precision into a region in which uncertainty has been the prevailing feature. His interesting work on the Kew temperature scale may lead to Kew methods becoming international.

The testing work of the observatory department has increased, and the department appears now to turn out " hall-marked " men, *e.g.* Wood, Simpson, and Gold, as well as " hall-marked " instruments. Two important discussions, by Dr. Chree, of terrestrial magnetism and of atmospheric electricity records, and their relation to meteorological phenomena, show that there is no likelihood of the reputation of Kew as a place of research suffering now it has lost its independence.

The few investigations mentioned above serve as examples of the work that is being done in the laboratory, but they tell nothing of the good influences exerted by the laboratory through the members of its staff on the councils and at the meetings of scientific and technical societies throughout the kingdom.

Although there will always be members of the public, and even Members of Parliament, who are unable to understand why any of the nation's money should be invested outside the circle of " small profits and quick returns," no one who is anxious that our country should stand shoulder to shoulder with its neighbours in the march of scientific and industrial progress can feel other than gratified that in establishing the National Physical Laboratory we have taken a step in the right direction.

C. H. L.

DR. EDWARD JOHN ROUTH, F.R.S.

BY the death of Dr. Routh on June 7, after a period of gradually failing health, a commanding figure in the recent history of English mathematics has been removed. Born at Quebec in 1831, the son of a distinguished British officer, he was educated in London at University College School, and subsequently studied mathematics under de Morgan at University College. He matriculated at Peterhouse in 1850, but did not drop his London connection, obtaining the gold medal in mathematics with the degree of Master of Arts in 1853, then a somewhat rare distinction. At Peterhouse he had Clerk Maxwell, who

soon after migrated to Trinity, as his rival in the same year; while Tait and Steele were undergraduates of the College, and Lord Kelvin (already Prof. W. Thomson, of Glasgow) was a junior Fellow.

Not long after taking his degree, in January, 1854, being senior wrangler, and bracketed with Clerk Maxwell for the Smith's prizes, he began the career of tuition of advanced honour men in mathematics, which was soon to lead to a unique reputation as a successful teacher. From 1858 to 1888 he had, in all, between 600 and 650 pupils, of whom the great majority graduated as wranglers, twenty-seven being seniors, while forty-one were Smith's prizemen; between 1861 and 1885, when he retired from this strenuous work at the age of fifty-four, he had all the senior wranglers as pupils, with but one exception near the end of the time.[1] The number of his pupils, which was for many years about 100, was not at all unprecedented; what was unique was the fact that for all this time he directed, almost without challenge, most of the intellectual activity of the *élite* of the undergraduate mathematical side of the University. This herculean task naturally demanded methodical arrangements, and the husbanding of his resources to the utmost. What he aimed at was to impart thorough mastery of the main principles of ascertained knowledge over the field of mathematics then cultivated at Cambridge; it was clearly out of the question to stray very far into the regions of nascent science in which ordered theory gradually evolves itself in response to concentrated and specialised effort. He was in the habit of claiming that this would follow spontaneously in the case of the mathematician born, once he had learnt mastery of the resources of the science; while even when it did not follow, the record in the legal and other professions of persons who had done well in youth in mathematical studies proved their supreme value as a deductive mental discipline.

His plan was to take small classes, each of about ten men selected to run together, and to maintain an average by catechetical methods. Those who could go faster than the average had extra material provided in the form of manuscript digests for study, and especially in the institution of a weekly paper of about a dozen problems, selected from recent examination papers, or abstracted from memoirs in the home and foreign mathematical journals. An element of competition formed a stimulus in answering these papers, while written solutions were afterwards at hand for study in cases of failure to unravel them. Looking back on those times, it might be thought that there was too much problem and too little sustained theory; but no one ever accused the standard of the problems selected of being lower than it ought to be, while, on the other hand, absence of some such rigid procedure would have rendered quite impossible that focussing of undergraduate mathematical activity and ambition in one place which was a main feature of the system. Men with further ambitions would struggle with Thomson and Tait's "Natural Philosophy" or with Maxwell's "Electricity," or with brilliant and stimulating courses of lectures given on growing special subjects by the more eminent mathematical physicists and thus learn that though in youth mastery may be rapid, yet at all times invention must be slow. It was, moreover, thus possible for the abler men to have time to spare to expand their outlook by taking up some other branch of knowledge as a relaxation from mathematics, or for joining in other activities of the University. Nowadays the field covered by the mathematical instruction offered at Cambridge is vastly wider than would have been conceived as practicable twenty years ago; but the question is still unsettled how far it is expedient to extend the preliminary undergraduate course into complex special theories.

Whatever may be thought as regards Dr. Routh's views on postponing special research in favour of thorough preparation, it could not be urged that he did not himself, notwithstanding his other absorbing work, set an example of what research might be. Many of his earlier papers, mainly in the *Quarterly Journal of Mathematics,* related to the dynamics of rigid solids, spinning tops, rolling globes, precession and nutation, and such like, and were distinguished by the development of methods relating to moving systems of coordinate axes, and to the differentiation of vectors such as velocity and momentum with regard to them. In another connection he applied the kinematics of special systems of coordinate axes moving along a curve to problems of curvature and torsion. The advantages of these methods in differential geometry have come again into recognition, as may be seen in such works as Darboux's "Théorie des Surfaces." Afterwards, arising out of his researches on dynamical stability, which will be referred to presently in more detail, there came a series of papers in the Proceedings of the London Mathematical Society on the propagation of waves and the analysis of complex vibrations in networks of interlacing threads and in other such laminar systems, leading up to a mechanical treatment or illustration of the broad general theory of harmonic analysis, principal periods, and related topics.

In the early 'seventies, the question of the possible explanation of steady, including apparently statical, relations of material systems by the existence of latent steady motions, such as the rotations of concealed flywheels or gyrostats attached to the system, was much to the fore. The fundamental problem as regards such representations is their degree of permanence; for a state of motion which falls away, however slowly, cannot be appealed to in elucidation of secular steadiness of relations. At a later stage the ideas of the subject were crystallised by Lord Kelvin in his British Association address, Montreal, 1884, entitled "Steps towards a Kinetic Theory of Matter," and in later addresses on cognate topics, mainly reprinted in vol. i. (Constitution of Matter) of his "Popular Lectures and Addresses," culminating in a way in 1897 in his gyrostatic model of a rotationally elastic optical æther.

It is thus not surprising that the Adams prize subject at Cambridge for the period 1875-7, announced over the signatures of Challis, Clerk Maxwell, and Stokes, should have been the search for "The Criterion of Dynamical Stability." This subject suited Routh's predilections exactly; and his classical essay, "A Treatise on the Stability of a Given State of Motion, particularly Steady Motion," composed, as he states in the preface, almost entirely during the year 1876, was the result. The greater part of the work in the essay is analytical, and is concerned with the discussion of the nature of the roots of the algebraic equation determining the free period of slight vibration of the dynamical system; but where it enters upon the discussion of dynamical principles, such as the criteria connected with the Energy and the Action, the essay moves in a high plane. In particular, the burning question of how adequately to represent latent, and, therefore, unknown steady motions, such as those of concealed flywheels or gyrostats attached to the system, is solved at a stroke by the famous theorem of the "modified Lagrangian function." It was established, in fact, that the presence of concealed steady motions does not fundamentally alter the standard mode of analytical specification of dynamical interaction developed originally by Lagrange, except in the one respect that the effective Lagrangian function

now involves terms linear in the velocity-components as well as quadratic terms. The procedure of Lagrange, evolved originally from the side of the Principle of Action, constituted the science of general dynamics by eliminating from the problem all variables the values of which are prescribed in terms of the remaining ones by relations of permanent constraint, thus reducing the dynamical analysis to the discussion of just as many quantities as are required to specify the state of the system. It gives cause for some surprise that nearly a century elapsed before the correlative step was taken, namely, the elimination from the analytical specification of the system of permanently steady or cyclic motions, as well as the permanent geometrical constraints above mentioned. In the hands of the analysts who treated the subject meanwhile, the requirements of the actual planetary and lunar theories were perhaps the main aim; it is only recently, and largely in the hands of the English school, notably Lord Kelvin and Clerk Maxwell, in later conjunction with Helmholtz, and building largely on the earlier work of W. Rowan Hamilton, that the subject of general dynamics has been welded into an instrument for the inductive, and in many cases speculative, exploration of physical processes in general. Anyhow, it will be evident how fundamental an advance in the principles of the dynamical interpretation of nature was involved in Routh's formulation of what he called the "modified Lagrangian function."

The problem thus solved by Routh with remarkable simplicity had already been some time in evidence. In the first edition of Thomson and Tait's "Natural Philosophy" in 1868, the equations of Lagrange had been applied in most effective manner to problems of motions of solids in fluid media, the energy function involved being determined in terms of the motions of the solids alone, and the fluid thus being *ignored* in the subsequent work. This procedure was soon challenged by Kirchhoff, as going beyond the existing conditions of validity of general dynamical theory; and a special justification for the case of motion in fluids was given by him on the basis of a Least Action analysis. Soon afterwards the same difficulty was pressed on Lord Kelvin independently by J. Purser, who also published a justification on more physical lines. This was, not unlikely, the origin of Lord Kelvin's general theory of "ignoration of coordinates," first published in 1879 in the second edition of Thomson and Tait's work, but which probably existed in manuscript anterior to Routh's essay. A report was once current that most of it was worked out in the harbour of Cherbourg, while his yacht was refitting, and the carpenters were all the time hammering overhead. This form of the theory, though more expressly suggested by the needs of physical dynamics, was less complete in one respect than Routh's, in that it did not bring the matter into direct relation with a single characteristic function (Lagrangian function of Routh, kinetic potential of Helmholtz), but simply obtained and illustrated the equations of motion that arose from the elimination of the cyclic coordinates that could be thus ignored.

Later still, Helmholtz, in his studies on monocyclic and polycyclic kinetic systems, which began in 1884 and culminated in the important memoir on the physical meaning of the Principle of Least Action in vol. c. (1886) of *Crelle's Journal*, developed the same theory more in Routh's manner, and built round it an extensive discussion of physical phenomena, so that on the Continent the whole subject is usually coupled with his name. Shortly before, the work of Routh and Kelvin had already been coordinated with the Principle of Action by more than one writer in England.

The most elaborate published result of Dr. Routh's scientific activity was the "Treatise on the Dynamics of a System of Rigid Bodies," which began as a thorough, though rather difficult, handbook in one octavo volume, but expanded in successive editions in a manner of which other classical instances readily occur to mind, until it became a sort of cyclopedia of the dynamical section of theoretical physics. In the course of an inquiry some ten years ago as to the reason why English mathematical physicists had so much practical command over the application of their knowledge, the mode of teaching in Cambridge came under review; and in particular this book was discovered by Prof. F. Klein, of Göttingen, who made arrangements for its introduction to the Continental public in a German translation, containing some brief valuable annotations such as the wide analytical outlook at Göttingen suggested. Especially was emphasis given to the great extension of the scope of abstract dynamics above described, with which Routh's name was associated, it is to be hoped permanently. Somehow the book does not seem to have attracted even yet much sustained attention in France.

Until lately, Dr. Routh's presence was a familiar and welcome one to residents in Cambridge. Though he never sought public positions, his services were in requisition in many ways, as Senator and Fellow of the University of London, as member of the University Council at Cambridge, member of council of the Royal Society, and in other activities; while he declined more prominent offices more than once. In society he was bright and attractive though somewhat retiring, simple, and entirely free from any suggestion of superiority. The respect and affection which he inspired in a long succession of distinguished pupils found expression on the occasion of his partial withdrawal from work in 1888, when at a remarkable gathering of judges, engineers, and men of science, his portrait by Herkomer was presented to Mrs. Routh, with many expressions of warm appreciation. His leisure he employed mainly in mathematical research, and in the preparation of a series of treatises on subjects of mathematical physics, of which the only criticism to be made is that his wealth of valuable material tended to convert them into cyclopedias rather than text-books. His last public action was to take the lead in opposition to the proposals for change in the system of the mathematical tripos at Cambridge. It is possible that he did not fully realise the altered circumstances of the time, and the insistent claims of other studies; anyhow, it will be matter for congratulation if the new arrangements work as well and as smoothly as did the older mathematical tripos during the long period when the practical direction was mainly in his hands.

J. L.

PROF. A. S. HERSCHEL, F.R.S.

THE death of Prof. Alexander Stewart Herschel, F.R.S., on June 18 will be deplored by many astronomers. Prof. Herschel was born in 1836, and was the second son of Sir John Herschel. He was appointed professor of physics at the Durham College of Science Newcastle-on-Tyne, in 1871, and was honorary professor and governor of the college at the time of his death, though he left Newcastle about twenty years ago, and resided with his brother, Col. John Herschel, F.R.S., at Observatory House, Slough, which was the home of his renowned grandfather, Sir William Herschel, and of his father. Prof. Herschel was elected a Fellow of the Royal Astronomical Society in 1867, and of the Royal Society in 1884.

Inheriting an illustrious name, Prof. Herschel also inherited the love for astronomy, the indomitable perseverance and capacity for work,